VOL. XII. NO. 8.—BOTANICAL GAZETTE.—AUGUST, 1887.

Vegetable parasites and evolution.[1]

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In the countless discussions concerning evolution which have followed the publication of Darwin's Origin of Species, zoölogists have gone farther than botanists in their efforts to explain the possible origin of higher forms from the lower. Botanists, as a rule, have contented themselves with a consideration of the ancestral relations of the orders of higher plants, but, until very recently, they have scarcely made any serious attempt to present a general scheme showing, from an evolutionary point of view, the relations of all the groups of the vegetable kingdom. This may be due either to their timidity—perhaps modesty is a better sounding word—or to their ignorance. If the latter, they have certainly been wise in avoiding unnecessary display of their ignorance; if the former, they can easily be pardoned, when one considers how large a part an aggressive audacity savoring of sensationalism has played in the formation of some schemes of development.

On abstract grounds alone, I presume that few botanists would object to the statement that all plants have been developed from simple ancestral forms, which were nearly related to some of the lower animals; but, when it comes to saying in anything like a definite way that certain existing forms have arisen from other lower existing forms or their immediate allies in some past epoch, and so on until the lowest form is reached, botanists may well insist that imagination should not be allowed too large a scope in supplying missing links. It is precisely in this point that zoölogists have an advantage over botanists. The palæontological record of lower animals is more complete than that of lower plants, so that, where the zoölogist might reasonably form an hypothesis, the botanist must rely more on his imagination, until, in the end, he finds himself in the possession of a chain composed to a considerable extent of missing links. As it is, if we would consider the evolution of plants, not getting much light on the progress of the lower forms from palæontology, we are compelled to trust largely to plants as we now

[1] Vice-Presidential address before Section F, A. A. A. S., New York, August 10, 1887.

find them, and to ask what are the inferences which we are permitted to draw from existing structures and conditions. I shall not attempt to offer any scheme of development, or to sketch a family tree whose roots are Protococci and bacteria and whose ripe apples are the genera of Phænogams, but shall restrict myself to some considerations concerning vegetable parasites and the inferences as to their possible origin which may be gathered from what we already know of their structure and habits; partly because this is a group of plants in which I am especially interested, and partly because the problems which they offer, even if they can not be solved at the present day, are, at least, full of suggestions.

In the first place, a word as to the different kinds of plants which are included among parasites. A parasite is usually defined as a plant which is unable to transform inorganic material into organic compounds, and which is consequently obliged to obtain its organized materials from other plants or from animals. The definition, in general, is an accurate one and correctly defines the vast majority of vegetable parasites which belong to the class of fungi. That they are strictly dependent on the organized materials derived eventually from other plants or animals is sufficiently evident when we consider that fungi are destitute of chlorophyll, the necessary agent in the assimilation of inorganic material. Of the parasites proper, we have two kinds: the saprophytes, which live on dead or inert matter; and the special, or true, parasites, as they are usually called, which can only grow on the tissues of living plants or animals. Whether the line between saprophytes and true parasites is sharply defined is a point which need not at present be discussed. It is enough to say that, as a rule, saprophytes grow more or less indiscriminately on dead organic substances, while the true parasites are generally limited to a single species of plants or to the species of a single genus or order. It is almost unnecessary to cite instances of the two kinds of parasites proper, since you will at once call to mind Penicillium and other common moulds which grow as saprophytes on an endless variety of substances; while we have as illustrations of true parasites the grape Peronospora, which grows abundantly on species of Vitis, and is occasionally found on Ampelopsis and Cissus, both genera of Vitaceæ, and the potato-rot fungus, sometimes found on the tomato, which belongs to the same order as the potato, and rarely on species of Scrophulariaceæ, a nearly related order.

The proper parasites do not exclusively belong to the class of fungi. You are familiar with the Indian pipe (Monotropa) and dodder (Cuscuta), which are our common representatives of parasites, which are found in a comparatively small number of orders of Phænogams. As chlorophyll is wanting in these plants, we are forced to assume that the parasitism is as complete as in fungi. You, also, will recall the mistletoes and the Gerardias, together with other members of the Scrophulariaceæ, which are not proper parasites in the sense in which we have already spoken, but may rather be called partial parasites; because, while they have chlorophyll and are to a certain extent able to transform inorganic into organic material, they still depend in part on material taken from other plants to which they are more or less closely attached. In the discussion of the evolution of parasites, the phænogamic parasites, however, are of comparatively little importance; because, by means of their flowers and fruit, they are rightly classed as belonging to, or closely related to, well recognized orders of Phænogams, and the question of the origin of the parasites themselves is not to be separated from the question of the origin of Phænogams as a whole, so that, in this case, we have only to account for the modification of the organs of vegetation whose greater simplicity may be explained by the loss of leaves and other assimilating organs which have become unnecessary to plants that have acquired the power of living upon the food assimilated by other plants. In short, as far as parasitic Phænogams are concerned, they may be regarded as degenerate forms of other Phænogams, for, in a plant, the inability to assimilate inorganic material should be regarded as a degraded condition in which the chances of survival are diminished unless some extraordinary provision is made for reproduction, which is not the case in Phænogams, whatever may be true of fungi.

Whether any proper parasites are to be found among algæ is a question on which there is a difference of opinion. For my own part, I am unable to recognize any proper parasite among algæ, although it is tolerably certain that a number of forms generally classed among algæ may be regarded as partial parasites. This point, however, can be better considered later on.

Let us next briefly consider the mutual relations which exist between parasites and their hosts—that is, the substances, dead or living, on which they are growing. At first

sight a parasite would seem to be purely destructive in its action; and that this is really the case is evident in the great majority of instances. When a piece of bread is attacked by the mould, Mucor stolonifer, its substance continually diminishes with the growth of the mould. We need not stop to consider the saprophytes, for the case of the true parasites is still stronger. There is a constant struggle between the rots, rusts and other true parasites and the hosts on which they are growing. Just so far as the fungus flourishes, so does the host suffer; and, if the conditions of temperature and moisture are favorable to the fungus, the host may be quite destroyed. If the conditions are not favorable to the fungus, the host may continue to grow, and the fungus may gradually disappear, or, at least, pass into a quiescent state. An instance of the sudden and complete destruction of the host is seen in the case of the bad epidemics of the potato rot, when whole acres suddenly rot and die. This is an extreme case. Other members of the Peronosporeæ attack young seedlings, some of which are destroyed, while others continue to grow, and may be said to throw off the fungus. From cases of the complete and sudden destruction of the host, either in its mature or seedling condition, we pass to parasites which are less virulent and whose action is more local. We have all grades of injury done to the host, from the destruction of the leaves and consequent diminution of the assimilating power, which may entail serious or fatal results; from the formation of circumscribed knots and tumors, which may cause destruction of the branches and, in course of years, the death of the plant; from fungi which attack the flowers or fruit and cause diminished reproductivity without injury to the vegetative powers, down to the insignificant distortions of scattered epidermal cells caused by Synchytria. But in all these cases the action of the parasite is destructive. We can not conceive that it is of the slightest benefit to the host. It robs the plant of the food which it needs for itself, and gives back nothing good in return.

We have, on the other hand, instances of parasitism in which it is claimed that the relation of parasite and host[2] is

[2] The word "symbiosis" was originally applied to all cases where different organisms were associated together in a community, and in this sense included the true parasites. The application has gradually been modified until, at the present day, symbiosis is generally understood to mean the association of plants with plants, or plants with animals, in such a way that the relation between them is one of mutual benefit, or in which there is, at least, no injurious action of one organism on the other. In this sense, as contrasted with true parasitism, the word is here used.

one of mutual benefit. To this condition the name symbiosis has been applied. The two most marked instances of symbiosis among plants are to be found in lichens and the fungus growth first called by Frank Mycorhiza.

The thallus of lichens, you will bear in mind, is composed of two elements: the green cells or filaments called gonidia, and the colorless threads or hyphæ. Any extended discussion of the algo-fungal theory of lichens would be out of place on the present occasion, and it is only necessary to say that I do not see why we may not consider the gonidia to be what they appear to be, viz., algæ, and the hyphæ fungi parasitic on the gonidia. Certainly, the opponents of the algo-fungal theory, in spite of all their attempts, have not, as yet, given satisfactory proof that the gonidia are produced from the hyphæ or the hyphæ from the gonidia; so that we are forced to regard them as two distinct entities. The strong point of the opponents of the algo-fungal theory has been that, if it is true that what is called a lichen is really a fungus parasitic on an alga, it is inconceivable that the alga should not be injured, or even destroyed, by the fungus. It is certainly a fact that the gonidia, or algæ, are not destroyed, and it has been assumed by both the advocates and opponents of the theory that the gonidia are not injured by the growth of the hyphæ, while some even go so far as to say that their growth is aided thereby. To account for this state of things, the advocates of the theory have advanced the view that in lichens we have a sort of mutual parasitism; and the statement has been made that "the hyphæ lie on the gonidia, and carry to them crude nutritive fluids, in return for which they receive a part of the assimilated material in the gonidia." But what good the gonidia can derive by having crude material brought to them by the hyphæ, if they must give back a part of the assimilated material to them, is not clear, since it is a well-known fact that the gonidia can, and very often do, live and flourish in a free condition, and are amply able to obtain all the nourishment they need without the help of the hyphæ, and at the same time can use for their own exclusive benefit all the assimilated material. On the other hand, it is known that the hyphæ are dependent on the gonidia for their development. The advantage to the gonidia is quite hypothetical; the advantage to the hyphæ is real; and it is, to speak mildly, a bad case of what the French call *un œuf pour un bœuf*.

The alleged proof that the gonidia are benefited by con-

tact with the hyphæ rests on laboratory cultures in which it is claimed that, if the germinating spores of lichens be brought in contact with pure gonidia, the hyphæ at once grow more rapidly, and the gonidia also begin to multiply. But this increase of the gonidia is not necessarily a sign that the conditions of growth have become more favorable. When the black knot fungus attacks a branch of the plum tree the parenchymatous cells increase, and a knot is formed; and the same thing occurs when branches of red cedar are attacked by Gymnosporangium macropus. Here the increased growth does not indicate an increased supply of food, but an irritation caused by a noxious parasite. The increased growth of normal cells in the presence of irritating foreign bodies is well known to both animal and vegetable pathologists, and is not interpreted by them to mean an improved condition, but rather an attempt to get rid of something harmful. The same explanation may be given to the lichen cultures. But cultures on microscopic slides in the laboratory surely should not be regarded as more conclusive than what is seen on a much larger scale in nature. One has only to compare the Chroolepus-forms which constitute the gonidia of Opegrapha with the same forms when free from the hyphæ of the Opegrapha to be convinced that they grow and fructify decidedly better when free than when shut up in the lichen thallus. They are neither benefited nor destroyed, but they are weakened and injured. The same is true of the cystococcoid or protococcoid gonidia of the larger lichens, which are more luxuriant when growing free on rocks and bark. It is impossible to regard the Stigonema gonidia, distorted and broken up by the hyphæ, as in a more flourishing condition than when free.

It seems to me that the real error of the supporters of the algo-fungal theory is not that they assume that the gonidia, the algæ, can support themselves and the hyphæ too, but that they assume that they are not injured thereby. In their attempt to show how a possible advantage to the gonidia might arise, they have not sufficiently regarded the palpably injurious action of the hyphæ. From the facts which I have given it is plain that they are injured, and, if the injury is less than in most cases of parasitism—which may be due to the fact that the hyphæ of lichens grow more slowly than those of other fungi—it is nevertheless an injury, and we must recognize in lichens not a case of symbiosis or mutual parasitism, but a case of true parasitism with a minimum of injury to the

host. In view of the facts, one can be an advocate of the algo-fungal theory without believing that there is a double parasitism.

In 1885[3] Frank announced the following discovery: that certain species of trees, especially Cupuliferæ, do not regularly obtain their food directly from the soil, but their roots are connected with the mycelium of a fungus by whose agency all the nourishment is transferred from the soil to the tree. He called this condition Mycorhiza, and described the fungus as intimately united with the inner cortex of the roots just back of the tips and forming a felt-like cap over the tips. He maintained that this union of mycelium and roots was of constant occurrence in the Cupuliferæ which he had examined, oaks, beeches, chestnuts, hazel-nuts and hornbeams, and more or less constant in Salicaceæ and Coniferæ. At a later date[4] he went further and stated that the Mycorhiza is a symbiotic condition which may perhaps be found in all trees under certain conditions; that it is found only where the soil consists of humus or undecomposed plant remains; that the fungus of the Mycorhiza conveys to the tree not only the necessary water and the mineral constituents of the soil, but also the organic material derived directly from the humus and decomposing vegetable matter; and that it is through the agency of the fungus alone that the tree obtains its food from the soil. If one could accept without reserve the conclusions of Frank, we have in Mycorhiza a clear case of symbiosis in which a fungus which lives as a saprophyte on vegetable mould is intimately united with the tissues of Phænogams on which it acts, not as a parasite but as a conveyer of nourishment. Unfortunately, the statements of Frank are, to a great extent, not confirmed by other competent observers. R. Hartig has shown[5] that the Mycorhiza condition is not at all necessary to the nourishment of trees even in Cupuliferæ, since he finds that, in many cases, roots of healthy trees are quite free from Mycorhiza, and, even in trees where there is a marked Mycorhiza of some roots, there are others quite free from it. He regards Mycorhiza not as a case of symbiosis comparable to that of lichens, as does Frank, but rather a case of proper parasitism, and Kamienski[6] states that, in the cases of Mycorhiza of trees which he has examined, he has always found evidences of injury done to the roots by

[3] Ber. Deutsch Bot. Ges. III, 128.
[4] l. c. III, XXVII.
[5] Bot. Centralblatt, XXV, 350.
[6] Bot. Centralblatt, XXX, 2.

the fungus, which he also regards as a parasite of a destructive nature. P. E. Mueller, to a certain extent, endorses Frank's views, as far as the Mycorhiza of beeches is concerned.

We are, on the whole, warranted in believing that the Mycorhiza condition is rather a condition of proper parasitism than of symbiosis in the case of trees. We still have the case of Monotropa Hypopitys, a small ericaceous parasite, in which Kamienski showed, as early as 1881, that the roots are surrounded by a mycelium, which, however, does not penetrate into the substance of the roots, as in the Mycorhiza of Frank. He considered that the fungus, in this case, was the medium of transfer of nourishment to the Monotropa, and did not agree with the then prevailing view that Monotropa itself was directly parasitic on the roots of other plants. We may safely consider that there is a symbiosis in Monotropa Hypopitys, and further investigation may show a similar condition in some other closely related phænogamous plants which are destitute of chlorophyll; but here the case is very different from that of the large trees, abundantly provided with assimilating organs of their own in which, if there is symbiosis at all, it certainly does not exist on the wholesale scale which Frank claims.

With regard to the symbiosis of plants and animals I will say but a word, for the subject is one which pertains to the domain of the zoölogist rather than to that of the botanist. The inherent objections against the probability that plants and animals should live in a state of symbiosis are less than in the case of symbiosis in the vegetable kingdom; because, in the former case, the plants in question belong not to the group of fungi, but are algæ possessing chlorophyll, or a modified form of chlorophyll. The symbiotic alga could not support itself; the animal, on the other hand, could support itself; and, bearing in mind the different products of assimilation and respiration in plants and animals, one could easily conceive the benefit which might arise from the combination of the two. Whether the combination really exists in many cases is not yet certain, because it too frequently happens that zoölogists do not agree as to whether the assumed alga is really an alga or a proper organ of the animal itself.

It becomes a question of authority, and a botanist is not in a condition to estimate the comparative merits of observations made by zoölogists. As far as I am at liberty to form any opinion at all, I should say that zoölogists were inclined

to accept, at least, a mechanical symbiosis of unicellular algæ and animals in a considerable number of instances. Whether the symbiosis is physiological as well as mechanical is a point on which more light is apparently needed.

The symbiosis of plants and animals is, perhaps, better to be compared with that of Nostocs with Hepaticæ and Azolla than with the condition which exists in lichens. Some of the recorded cases show clearly a mechanical symbiosis, even if others be regarded as merely accidental and temporary unions of different organisms. Whether the symbiosis here is physiologically of advantage to both organisms is doubtful. The Nostocs are certainly not injured, and they may derive benefit from the shelter afforded. It will not do to go too far in this direction, however, because we should, at length, be forced to speak of symbiosis in cases where Nostocs grow in crevices of rocks, which would be absurd.

I have dwelt somewhat at length on the subject of symbiosis because, as it seems to me, botanists have gone too far in assuming a beneficial action of the parasite on the host in many cases where not only no direct benefit can be proved to exist, but where a closer examination shows that an injury is really done, although it may be slight. In short, symbiosis, as distinct from true destructive parasitism, is not the comparatively common condition in the vegetable kingdom which it is generally supposed to be by those whose opinion is worth considering; for we need not regard those writers who, seeing in symbiosis a charming instance of domestic felicity and concord with which they can point a moral and adorn a tale, have given to the public essays whose only proper place is on the shelves of a Sunday School library. Accepting the existence of symbiosis where both members are chlorophyll-bearing plants, we must still believe that, with rare exceptions, the cases where one member is a fungus should be referred rather to the class of true parasites, in which the advantage is altogether on one side.

If we turn now to the question of the origin of vegetable parasites we find ourselves in a dilemma. Certainly, the parasites could not have originated before the plants and animals on whose remains or in whose tissues they live. On the other hand, accepting the law of evolution, that the more complex forms are developed later than the simpler forms, the parasites must have preceded the forms on which they prey. The paradox is, however, more in words than in reality. We can only suppose that our present parasites

have existed from early times, but were not always parasites. The question might arise here, What do we mean by higher and lower forms? The terms are elastic, and one sometimes suspects that they have been stretched and twisted to suit the necessities of individual writers. It is not quite plain, for instance, why we should say that the giant kelp of the Pacific, Macrocystis pyrifera, with its branching stems several hundred feet long, furnished with innumerable leaves and air-bladders, is less highly organized than the small frondose hepatics, like Riccia, or such mosses as Phascum. There is one point on which all botanists would probably agree in speaking of high or low organizations, viz.: that complications of the reproductive apparatus indicate a high organization, however simple the vegetative organs may be, and that as we advance higher in the scale we find more and more numerous embryonic conditions which represent free conditions of less highly organized plants.

Throwing out of consideration the phænogamous parasites for the reasons previously given, there is no doubt that the immense majority of vegetable parasites belong low down in the scale of development, and we can infer from the simplicity of their reproduction that they originated at an early period. Other things also point in the same direction. In the class of fungi, although the sexual reproduction is of low grade, it embraces a number of different types, and, as far as non-sexual modes of propagation are concerned, although it may be said that they only indicate an effort on the part of the plants to adapt themselves to peculiar conditions, fungi are far better provided than any other plants. We are, perhaps, at liberty to suppose a remote origin from the large number of species of fungi now in existence, and, in this connection, a few statistics may prove of interest. The question is frequently asked whether the species of fungi are more numerous than those of Phænogams. It is safe to suppose that they are, although it is not true that more species of fungi have already been described. The systematic study of fungi in Europe and North America is of so much more recent date than the study of the Phænogams of those two continents, while the fungi of a large share of the earth have scarcely been studied at all, that a comparison of the described species of the two classes fails to give a correct estimate of the real numbers. The reason for supposing that the species of fungi are more numerous than those of Phænogams is founded on the fact that, in countries where fungal

flora has been most thoroughly studied, we find few species of Phænogams which are not already known to be attacked by some special parasite, while the majority of species serve as hosts for a considerable number of species of fungi. A few figures will show this point clearly. In his treatise on the fungi which attack the species of Vitis, published in 1879, Pirotta enumerates one hundred and four species of parasites. Between ten and twenty of these are fungi not found on Vitis alone, but this number is more than counterbalanced by species peculiar to Vitis which have been described since 1879. It may be objected that some of the forms called species by Pirotta are probably merely stages of some of the other species enumerated. Admitting that this is possible, and even probable, if we deduct half, or even two-thirds, which is liberal to the last degree, we still have thirty to fifty species of fungi, at the lowest estimate, which are peculiar to six species of Vitis, the number of species of the genus included in Pirotta's observations. I have little doubt that the real number of species of fungi peculiar to the genus Vitis is much larger than the estimate I have just given. If the relative number of species known to occur on Vitis is greater than that of those known on most other genera, it is due rather to the fact that, from their importance in horticulture, they have been more carefully studied than because other genera are less frequented by special fungi.

The province of Venetia is probably no richer in fungi than other parts of the world; but, as it is of small size and is the residence of a considerable number of mycologists, its flora has been more thoroughly studied than that of this country, and we can obtain a more accurate view if we examine statistics of the Venetian flora. Cuboni and Mancini enumerate sixty-five species of fungi which occur on the chestnut, and over three hundred species on Quercus, including three native species of that genus. If we deduct a large number for species which are not found exclusively on these two genera or which are merely secondary forms of other species, we still have a considerable number of fungi to a small number of Phænogams. Turning to the American flora, we find that the species of a genus as erratic as Sarracenia are not without their proper parasites; for on four species of Sarracenia we already know four species of fungi, three of which are peculiar to the genus. The list of fungi which grow on oaks in the United States includes between five hundred and six hundred species. The greater part, however, are not

peculiar to oaks, and, as the synonymy of the species is much confused, the exact number of fungi known on all our oaks can not be given exactly. On Quercus alba fifty-seven and on Q. tinctoria forty-six species are reported, about a quarter, or possibly a third, of which are probably peculiar to those species.

We can start with the postulate that vegetable parasites must have originated at an early epoch and must have been derived from non-parasitic forms. What forms? Here we enter upon the field of pure speculation. It can hardly be supposed that we shall ever know what was the earliest form of life. It may have been some protoplasmic structure which was neither strictly vegetable nor animal. Probably the earliest forms of undoubted plants were unicellular forms like Protococcus. The term Protococcus, as used at the present day, includes some forms which are claimed by zoölogists; whether rightly or wrongly is a question which need not concern us, for some Protococci are certainly plants. The Protococci are simple green cells which multiply by division into two, and so on, and which, at times, also produce in their interior zoöspores which escape and form new individuals.

How other chlorophyll-bearing plants might have arisen from Protococci we can not stop to consider, and we can only touch upon the possible origin of the colorless parasites. A vegetation consisting of simple forms like Protococci once established, there is no reason why there might not quickly have followed parasites of the order Chytridiaceæ the species of which abound at the present day in both salt and fresh water. The simple forms of the order consist of colorless cells which produce in their interior colorless zoöspores, which escape and attach themselves to submerged plants and animals.

The step from Protococcus to Chytridium is slight. We have only to suppose that a Protococcus has acquired the power of attaching itself to other Protococci or to low animals and has gradually lost the chlorophyll, which is no longer of service to a plant in a position to absorb nourishment directly from living organisms. Other natural changes would be the development of processes for attaching the Chytridium to the host or for enabling it to penetrate the walls of the host so that the parasite could make its way into the interior. In short, it is probable that, at a very early epoch, true parasites existed essentially like our present

Chytridia. If the first plants were marine, it is altogether likely that the first parasites were Chytridia, if we can judge by present conditions. In the present age comparatively few species of fungi grow in salt water. The few that we have belong principally to the Chytridiaceæ and are abundant enough on the marine algæ of all groups. Most of the other marine fungi are forms like Leptothrix, which may rather be regarded as degenerate forms of Nostocs or Schizophyceæ than as forms derived from anything like Protococcus or Chytridium. It is certainly true that there are very few species of fungi higher than Chytridium or Leptothrix, if one can call Leptothrix a fungus, found on strictly marine plants. There are a few, however, and on the New England coast the stipes of the digitate Laminariæ, while yet submerged, are attacked by a species of Sphærella belonging to the Pyrenomycetes.

Whether the filamentous and higher forms of parasites have been derived from the simple Chytridia is not easy to surmise. Among existing Chytridiaceæ we have a series of genera in some of which there are simple rhizoids, and in others, like Cladochytrium, a well-developed mycelium. Furthermore, the species of, at least, three Cladochytria have lost the aquatic habit, and live in the tissues of Iris, Menyanthes and Sanicula. In Polyphagus, Nowakowski has also observed a conjugation of the mycelium of two individuals. Admitting the fugitive character of the mycelium of Chytridiaceæ, there is still no reason why the filamentous fungi might not have developed from species of this order. The zoöspore-bearing cells, as the parasite lost its aquatic habit and became aerial, might naturally be transformed into sporangia with non-motile spores, like those of Mucor, and, as it acquired the power of growing in solid tissues, one of the conjugating cells would advantageously be developed into a pollinodium, and we should then find oösporic forms. But it is hardly worth while continuing the chain of possibilities further in this direction.

As I have said, it seems to me not unreasonable to suppose that true parasites may have originated at a very remote period primarily from non-parasitic plants. But we must also consider another question. Is it not more probable that saprophytes were first developed and from them arose the true parasites? The line between saprophytes and true parasites is not well defined among existing plants. Some species might, with sufficiently good reason, be placed in either class, for what

are called by Van Tieghem facultative parasites may live ordinarily as saprophytes and yet at times live a truly parasitic existence. The great majority of fungi are saprophytes, and De Bary has shown[7] in an instructive way how Peziza sclerotiorum, during a part of its existence, is a saprophyte, and becomes later a true parasite. The germinating spores will not penetrate the living cells of the carrot on which the mature forms of the fungus is found, but live a saprophytic existence for some time. After they have attained a certain growth and strength they are then able to make their way into the carrot, which they destroy. The mechanism is as follows: after a certain time the saprophytic hyphæ excrete an oxalate which is able to destroy the superficial cells of the carrot with which the hyphæ may come in contact, and the fungus then makes its way into the plant. It is probable that a considerable number of saprophytes may act in the same way as Peziza sclerotiorum, and it is not impossible that a good many existing saprophytes are developing into parasites, and, if the present state of things correctly represents what has always been going on, it would lead us to believe that the saprophytes first came into existence and the parasites followed. Since actual knowledge is out of the question, one can take either theory without denying the other *in toto*. The probabilities seem to me to favor the origin of Chytridia from Protococci, if we regard the morphological rather than the physiological side of the question. How far the first Chytridia were true parasites rather than saprophytes may be questioned. Decidedly, the majority of the living species, I should say, are parasites; but in some the parasitism is not well marked, and they may be conveniently called epiphytic.

Still another possibility must be considered. May we not suppose that the first living beings were protoplasmic bodies, neither plants nor animals, or both, if you please, and from them parasitic and non-parasitic plants were simultaneously developed? Orders like Myxomycetes might perhaps lead us to suppose that this view was the true one. But it may be assuming too much to suppose that Myxomycetes are plants at all. If they are plants, they have remained in a low condition, and have no offshoots represented by higher forms of plants. There appears to be only one way to find out whether a given structure is a plant or an animal, and that is to see whether it is described in zoölog-

[7] Bot. Zeit., 1886.

ical or botanical manuals. Unfortunately, this does not help us in the case of the Myxomycetes. We can safely say, however, that the more highly developed parasites have not been developed from Myxomycetes, and there is very little to lead us to believe that parasitic and non-parasitic plants were simultaneously developed from primitive protoplasmic structures.

It has already been stated that phænogamic parasites should be regarded as degenerate forms of other Phænogams. Their line of development is not through the parasites of the class of fungi. If one is willing to believe that the first parasites were Chytridiaceæ, or something very much like them, from which it is possible some of the filamentous zygosporic and oösporic fungi have been gradually developed, he is not, however, forced to believe that such a course of development is probable as well as possible. The class of fungi is not an homogeneous one. It is rather an assemblage of forms which have certain common physiological resemblances, but marked morphological differences. When one regards fungi as a single class of plants, and attempts to trace a clear connection between the highest and lowest members, he finds numerous gaps which can not well be filled. A general parallelism, however, exists between chlorophyll-bearing algæ and fungi, and one is forced to ask whether the order of development has not been from the lowest to the highest algæ—the class of algæ being more homogeneous than that of fungi—and whether the fungi have not arisen, not from any one primitive group of algæ, but from different groups of algæ at different periods in the progress from below upward. This view seems to be more in accord with existing facts than any other, and brings phænogamic parasites into harmony with the rest. If the phænogamic parasites may be regarded as derived directly from other Phænogams, so Chytridiaceæ may be supposed to be derived from Protococcaceæ. It may be that some of the zygosporic and oösporic fungi have come from the ultimate development of Chytridiaceæ, but it is more natural to suppose that the greater part of them are direct derivatives of zygosporic and oösporic algæ. Special applications of this theory would lead to so many technical details that they must be omitted on the present occasion. In general, if the theory is accepted, we should expect that the fungi first derived from any group of algæ would exhibit the characteristic modes of reproduction. In the sexual reproduction

both groups are much alike, and if there are fungi at the present day whose reproduction is different from that of any algæ, it is because the reproduction has assumed more and more a non-sexual character, until, as in some groups of what are called higher fungi, sexuality has quite disappeared, as is supposed to be the case in Basidiomycetes. It is sometimes said that non-sexual modes of reproduction always precede the sexual. This is true only to a certain extent. It may be true, for instance, that in the earliest forms which had zoöspores the zoöspores were at first non-sexual, and afterward acquired the power of conjugating. But in fungi, where we have more non-sexual forms of reproduction than anywhere else, they must, in most cases, be regarded as secondary and degraded, not primary forms. Fungi are plants which depart more and more from what we may call typical plants. When we speak of higher plants we mean those in which the organs of assimilation and sexual reproduction exhibit a high degree of differentiation. When we speak of higher fungi, however, we refer to forms in which the vegetative organs are represented merely by a system of colorless threads, and in which the sexual reproduction is seldom well marked, if it exists at all, and they can be called high only in the sense that their numerous and often complicated modes of non-sexual reproduction are better developed than in what are called the lower fungi. In the struggle for existence among the higher plants those succeed best which are best able to assimilate crude material in the growing season and have the largest provision of seeds and reservoirs of assimilated food to carry them over the season of rest. In the struggle for existence among fungi, although there is an advantage if the mycelium is able to assume an indurated condition, like the sclerotia, at seasons unfavorable for growth, it is of much greater importance that there should be a variety of reproductive bodies, some of which, at least, are light and easily transported, while others are denser and better able to endure extremes of temperature and moisture, so that the fungus may be able to take advantage of any chance which may arise should the proper host be present. How well they are able to take advantage of temporary favorable conditions is shown in the rapid spread of epidemic diseases caused by fungi.

But it is better not to pursue the subject further. What I have already said will, I fear, appear to you too vague and uncertain; for the balancing of possibilities, although par-

donable in philosophy, should not be carried too far in natural science. Of course, no celebration of our national anniversary is complete without a balloon ascension, and the more gas the better, provided the aeronaut, or, as the papers generally call him, the professor, only lands safely. So our society sends up its annual balloons in the shape of addresses in which the professors are allowed to soar above, though not out of sight of, facts. But they must not remain too long up in the air, and the gas for their balloons should be generated in the laboratory of experience and study. In their every-day work, it seems to me that the attitude of botanists, at the present day, is the correct one. Following the prevailing tendency in business affairs, the question they ask of plants is not so much, "Who is your father, and where did you come from?" as "What can you do?"

The identity of Podosphæra minor Howe and Microsphæra fulvofulcra Cooke.

MARTHA MERRY.

(WITH PLATE XI.)

About two years ago, while making a study of the American forms of the genus Microsphæra, the *M. fulvofulcra* Cooke came to my notice. The specimen examined was from the Ellis collection of North American fungi, No. 1,321. The so-called species is described in an article on "Californian Fungi," by Rev. J. E. Vize, in *Grevillea*, vol. v, p. 110, where it is said, "asci not seen." Examination of mature specimens shows clearly a single ascus in each perithecium, thus placing it in the genus Podosphæra. It agrees with the description of *Podosphæra minor* Howe, thus necessitating the cancellation of *Microsphæra fulvofulcra* Cooke.

Several specimens of the so-called Microsphæra fulvofulcra from different localities have been examined and compared with the original specimen of *Podosphæra minor* Howe. In all essential characteristics they agree. There are slight variations, arising probably from differences in hosts, vigor of growth, locality, age, etc.; but these variations are not marked. In many cases on the same leaf intermediate forms may be seen which unite the characteristics of the different specimens. They may all be embraced in the following description, a part of which is quoted from Mr. Howe's original description in *Bulletin Torrey Bot. Club*, v, p. 3:

"PODOSPHÆRA MINOR *Howe*. Conceptacles scattered or crowded; appendages 10–20, as long or a little longer than the diameter of the conceptacles.[1]—Leaves of Spiræa."

Mycelium amphigenous, dense on upper surface of leaf, sparing, arachnoid below.

Perithecia amphigenous, usually gregarious, and abundant above, scattered on under surface, reticulated, ranging from .071 to .0968 mm. in diameter, often .08.

Appendages 8–22, or even more, often 12, 17, 16, 21, from ½–2 times the diameter of the perithecium, septate usually 3–4 times, rarely 7, branching dichotomously 3–4 times, divisions sometimes divaricate, smooth or hyaline, color brown, shading from base to last septum, branches and last cell transparent, ultimate lobes usually merely turgid, occasionally revolute.

Ascus oval from .068×.045 mm. to .065×.0732 mm.

Spores 6–8, irregular in form, granular, .00726×.0209 to .0197×.0141 mm. Usually filling about one-half the ascus cavity.

The specimens which have furnished the essential material for the notes recorded in this paper, and some of which, accompanied by their respective numbers, are figured in the plate, were derived from the following sources:

No. I. *Podosphæra minor* Howe, on Spiræa tomentosa, from herbarium of E. C. Howe; a part of the original material from which the species P. minor was described.

No. II. "*Microsphæra fulvofulcra* Cooke," "on Spiræa salicifolia, from Providence, R. I.; No. 204; collected by N. M. Mason." Described in *Grevillea*, v, 110. Dr. Harkness, from whom it was received, says it is a part of the original material from which the "*M. fulvofulcra* Cke." was described and named. The collection number, "204," on the label of II is, moreover, identical with the number accompanying the description in *Grevillea*. The appendages are somewhat fewer and longer than in I, and the spores smaller.

No. III. "*Microsphæra fulvofulcra* Cooke," from herb. of Dr. Harkness; communicated by J. B. Ellis; on Spiræa tomentosa, probably originally from E. N. Amer. The under leaf surface of the host is strongly hirsute. The specimen is characterized by a less dense mycelium than I and II, a greater number of appendages (often 21), which are shorter, with heavier branches, presenting in all respects a more vig-

[1] The remaining part of the description has resulted from careful examination and measurement of a sufficient number of specimens to give a basis of characterization.

orous growth. On the same host-specimens forms were found with fewer appendages (12), in which case they are longer, with more frequent septa, thus approaching No. II.

No. IV. "*Microsphæra fulvofulcra* Cooke," ex herb. W. A. Kellerman, on Spiræa tomentosa; Fairfield Co., Ohio, Oct., 1882; the same as 1,323 of the Ellis collection of North American fungi. This form is almost identical with No. I. Whatever slight distinctions there may be bring it nearer No. III. The branching approaches III. The septa are less distinct.

After a careful review of this somewhat important material, it seems conclusively demonstrated that these forms are the same. In a note in the *Journal of Mycology*, June, 1885, Mr. Ellis referred to the error in his "North American Fungi," and rejects the species *Microsphæra fulvofulcra* Cooke. He also says that the error fallen into by Cooke has been repeated by Dr. Winter in his Exsiccati no. 3,045. The specimens examined agree with the description of forms on Spiræa tomentosa in "Notes on the Am. Forms of Podosphæra," by F. S. Earle, published in BOT. GAZETTE for February, 1884. Whether they should be known as *P. minor* H. or as *P. Oxyacanthæ* DC., as held by Mr. Earle, I am not prepared to say. At all events, they are clearly the same as his form, numbered (4).

To substantiate the above statements the accompanying camera-lucida drawings are offered. The numbers on the plate correspond to those in the preceding notes.

PLATE XI.—I. *Podosphæra minor* Howe, on Spiræa tomentosa, from E. C. Howe. II. The "*Microsphæra fulvofulcra* Cke.," on Spiræa salicifolia, Providence, R. I.; the "204" of the Harkness herb. III. "*M. fulvofulcra* Cke.," on Spiræa tomentosa, from Harkness herb. IV. "*M. fulvofulcra* Cke.," on Spiræa tomentosa, Fairfield Co., Ohio, from W. A. Kellerman; the same material as the "No. 1,323" of the Ellis Coll. "N. Amer. Fungi."

a = perithecium × 85 diam. b′ = appendage × 375 diam.
b = appendage × 85 diam. d = escaping ascus × 85 diam.
c = broken perithecium × 85 diam. d′ = ascus × 375 diam.
e = spore × 375 diameters.[2]

[2] Dr Peck, of the N. Y. S. Museum, forwarded two sets of specimens of the Podosphæra discussed in this paper, which were collected in Eastern N. Y., but arrived after Miss Merry had completed her work and left Ithaca. Several preparations of each were examined by myself, however, and measurements were recorded. The results are worth mentioning, because they are directly in the line of certain conclusions in the above paper, viz.: that differences of host or position occasion variations in the perithecia and the appendages of the parasite. In specimens from Spiræa salicifolia the appendages of the perithecia were from 9 to 12 in number, and in length from 1¾ to 2 times its diameter; spores 8 (and either 7 or 8 in the next). This clearly approaches the characters of the specimens on S. salicifolia already figured under II. Those on Dr. Peck's specimens of Spiræa tomentosa had from 16 to 19 or more appendages—in one case 27—whose length was from ¾ to 1¼ the diameter of the perithecium, thus agreeing with specimens on S. tomentosa figured in this paper. Furthermore, the specimens from the under surface of the S. tomentosa leaves had fewer (often only 12-15) and longer appendages than those on the upper side.—W. R. DUDLEY.]

BRIEFER ARTICLES.

Zannichellia palustris L.—Prof. Stanley Coulter's account, in the May number, of a certain pond and its contents is quite interesting to us in Montana. All the more so because we have a remarkable spring near Great Falls, known as the Giant Spring. This spring really seems to be the outlet of an underground river, the flow of water is so great and so strong. It discharges immediately into the Missouri river, midway between the Black Eagle and the Coulter Falls, and has a river frontage of 500 feet. Upon a future occasion I hope to make some mention of the various forms of plant life therein found. Zannichellia palustris grows there in abundance, and may be found in flower from May to September, and yet the temperature of the water is only about 52° F., and does not seem to vary with that of the atmosphere. So far as my own observations have gone, the stems of these plants with the flowers and fruit when growing in this spring are nearly always buried in sand, only the slender grass-like leaves waving above. And yet, covered up and packed in fine mud and sand as these plants are, their essential organs perform their functions unfailingly, and a prolific crop of fruit may be found each season. When the flowers are covered up in this manner they are always pale, often white, tinged with flesh color, but when growing exposed to the light are olivaceous and the covering of the nutlets is thicker and stronger.—F. W. Anderson. *Helena, Montana.*

Coloring the nuclei of living cells.—The most interesting fact brought out in my work at Tübingen is the fact that several aniline colors have the property of coloring the nucleus of many plant cells without killing them. That the living nucleus can be stained has been demonstrated by several observers in the case of animal cells,[1] but as far as I now know it has not hitherto been observed in plant cells. Though the work is not yet completed, it will perhaps be interesting to give briefly some of the processes by which the results were obtained, and some of the objects employed.

The first color used was dahlia, a violet-purple pigment by whose aid Lavalette[2] had succeeded in coloring living spermatozoa and the nuclei of sperm-cells. The most favorable object so far found by me is the nucleus of the cells of stamen hairs of Tradescantia. T. Virginica was principally used, but other species gave equally good results. Hairs should be chosen from young buds, as these are perfectly colorless, not having developed the colored cell-sap of the older hairs. The sepals and petals are removed, and the stamens thus exposed are plunged into an aqueous solution of the

[1] See Pfeffer, "Uber Aufnahme von Anilinfarben in lebende Zellen," Unters. aus dem bot. Institut, Tübingen, 1886. Also, Strasburger, "Botanisches Practicum," fourth edition.

[2] Strasburger, Bot. Pract., fourth edition.

dahlia. After an immersion of from half an hour to three or four hours, or even much longer, depending on the strength of the solution, it will be found that in many cases the nuclei are more or less deeply colored; and that the cell is not killed is evinced by the continuance of the protoplasmic streaming. It is quite surprising to see how deeply the nucleus is often stained without killing the cell. A nucleus so colored appears perfectly normal, there being no distortion or change beyond the change in color. As yet I have not studied especially what parts of the nucleus are colored, but it appears to be the nucleolus and microsomes only, as in the case of cells that have first been killed and then stained according to the ordinary methods.

Among other objects that have given more or less satisfactory results were the hairs from the base of the perianth of Lilium bulbiferum; stamen-hairs of Aspleodelus albus; leaves of Elodea Canadensis and Vallisneria spiralis; root-hairs of Trianea Bogatensis, Cucurbita Pepo, Tradescantia zebrina; spermatozoids of Chara and a fern (probably Blechnum). In all cases cells were chosen in which there was evident protoplasmic movement, in order that there might be a certain means of determining whether or not the cell was still living.

Similar and usually quite as good results were also obtained with mauvein and methyl-violet, both colors closely resembling dahlia. Usually a .1% solution was made, and this diluted with from 50 to 1,000 parts of water, according to circumstances. Some doubtful results were obtained with other colors, but too uncertain to warrant recording.—Douglas H. Campbell, *Tübingen*.

The absorption of aniline colors by living cells.—About a year ago Pfeffer[1] published the results of a rather extended series of experiments showing that, contrary to the ordinarily accepted idea, various aniline colors can be absorbed in large quantities by living cells. I wish here merely to call attention to some easily made but instructive experiments bearing on the subject. Pfeffer's experiments were mostly made with methylen-blue and methyl-violet, though numerous other colors were also tried. Among colors not employed by him I found that dahlia and mauvein, both very similar to methyl violet, were quite as good and acted much in the same way. The yellow color chrysoidin also gave good results. No very satisfactory results were obtained with red pigments, though in some cases safranin, tropæolin and fuchsin gave tolerably good coloring, but either it was too diffuse, or the cell-wall was more deeply colored than the contents.

With methylen-blue either the cell-sap is colored, often very intensely, e. g., root-hairs of Trianea Bogatensis, or a precipitate is formed in the cell-sap, e. g., Spirogyra. If vesicles of tannic acid are present, as is the case

[1] Untersuchungen aus dem botan. Institut in Tübingen, 1886. "Über Aufnahme von Anilinfarben in lebende Zellen."

in Zygnema, these are colored dark blue. Methyl-violet, dahlia and mauvein color the protoplasm and nucleus, and are specially valuable in the study of the latter. In some cases they are also precipitated in the cell-sap. Chrysoidin appears to color only the protoplasm. The following are some of the objects that were used: root-hairs of Trianea Bogatensis, Cucurbita, Tradescantia zebrina; stamen-hairs of various species of Tradescantia; Spirogyra spp., Zygnema spp.; roots of Lemna minor; leaves of Elodea (Anacharis) Canadensis, Vallisneria spiralis; pollen-tubes of Hemerocallis spp., Tradescantia Virginica, Scilla spp.; spermatozoids of Chara.

The objects are placed in a solution varying from .002 % to .001 %, varying with the nature of the cell-wall and the time of immersion. Root-hairs are usually especially delicate, and the solution should be very dilute or the immersion very brief.

In most cases objects were selected where there was marked protoplasmic streaming, as this is the best means of determining whether the cell is alive or not. It is surprising how deeply the protoplasm or nucleus may be stained without materially affecting the streaming. For a demonstration of the staining of the protoplasm the root-hairs of Trianea were found to be specially favorable on account of their large size and the rapid streaming, as well as the readiness with which the color is absorbed.—DOUGLAS H. CAMPBELL, *Tübingen*.

H. W. Ravenel.—Henry William Ravenel died at Aiken, S. C., July 17th. This is indeed sad news to all American botanists, for among their number there was none more respected and beloved and few whose scientific work covered so long a period. He was born in the parish of St. John's, Berkley, S. C., May 19, 1814. After receiving the usual high school training he entered the South Carolina College, and graduated with distinction in 1832. He then became a planter, and resided at St. John's for twenty years. In 1835 he was married to Miss Elizabeth Gilliard Snowden, of St. John's. His wife died in 1855, leaving a family of six children, four of whom still survive. In 1858 he married Miss Mary Huger Dawson, of Charleston, who, with five children, all daughters, survives to mourn their irreparable loss. In 1853 he removed to Aiken, S. C., where the remaining years of his life were spent. Although an active worker until the close of his life, when he suffered from a long illness following an attack of apoplexy, he was, unfortunately, for many years, afflicted with a deafness which was, at last, so great that he could hardly converse with strangers.

Mr. Ravenel was born with a fondness for natural history, and he pursued his studies in botany with enthusiasm during the whole of his long life. As a young man he explored minutely the region about St. John's, and he was equally active after his removal to Aiken. There was not a group of plants, no matter how small, which escaped his observation.

It is doubtful whether any other American botanist has ever covered so wide a range of plants. He not only studied critically the phænogams of South Carolina, but he collected and studied, as far as it was possible at that time and in a region remote from large libraries, mosses, lichens, algæ and fungi. But he was not an ordinary collector, heaping up rough material to be exchanged for specimens to be counted rather than studied. He was a most accurate observer, and always noted the habits and peculiarities of what he collected. He discovered a surprisingly large number of new species of cryptogams, besides a few new phænogams. Probably no person had so complete a knowledge of the cryptogamic flora of the southern states as he, and, for a long time, he and his friend, the late Rev. M. A. Curtis, of North Carolina, were practically the only Americans who knew specifically the fungi of the United States. Their interest in fungi brought them into correspondence with Berkeley, Montague, Fries and other leading mycologists of Europe of that time, and the name of Ravenel became well known abroad as well as at home.

His deafness made it impossible for him to accept any position which involved class instruction, and he was too modest to seek public preferment. The only occasion on which he accepted a government appointment was in 1869, when he was appointed botanist to the commission which, under Prof. Gamgee, was sent to Texas to investigate the cattle disease, and on his return he published a short report on the fungi of that state. At one time he was the agricultural editor of the *Weekly News and Courier*, and at the time of his death he held the position of botanist to the department of agriculture of South Carolina. He was an honorary member of a number of scientific societies of this country and Europe, and in 1886 the degree of LL. D. was conferred on him by the University of North Carolina.

As a writer, he was not voluminous, but the appended list of his works includes several valuable papers. They all show thoroughness and an active mind which went beyond mere descriptions and inquired into causes as well as results. The best known of his works is the "Fungi Caroliniani Exsiccati," in five volumes, the first of which appeared in 1853, and the last in 1860. This is the first published series of named specimens of American fungi of which only thirty copies were issued. Probably he intended to continue the work, but the breaking out of the war and the unsettled state of affairs which followed made it impossible to continue the series. At a later period he issued a second series in connection with the English mycologist, M. C. Cooke, under the title "Fungi Americani Exsiccati," of which eight centuries appeared from 1878 to 1882. The species of this second series were collected principally in South Carolina, Georgia and Texas. Apart from the publications which bear his name, if we would correctly estimate his contributions to American botany, we must also include the very numerous notes

and comments furnished by him to other writers, through whose pages they are scattered, a monument to his liberality and freedom from professional jealousy as well as to his industry and acuteness.

The name of Ravenel will be perpetuated in the genus Ravenelia of the Uredineæ, a genus so peculiar in its character that it is not probable that it will ever be reduced to a synonym. There are also many species of cryptogams named in honor of him as their discoverer. To those who have ever known him as a friend or correspondent, nothing is needful to keep his memory ever fresh. A devoted husband and father, a citizen whose life was full of kindness to all about him, a correspondent whose letters, overflowing with friendly feeling as well as information, were always a pleasure, a botanist inspired by a true love of nature and always guided by the spirit of investigation, we might call his loss irreparable had he not bequeathed to us an example of what a botanist should be. He was one of the pioneers of cryptogamic botany in this country. May those who follow be guided by the same spirit.

The following list includes all the works of Mr. Ravenel on botanical subjects as far as at present informed:

An enumeration of some few phænogamous plants, not heretofore published as inhabiting South Carolina, found in the vicinity of the Santee canal. *Charleston Medical Journal and Review*, iv, 32–38, 1849.

Contributions to the cryptogamic botany of South Carolina. *Charleston Med. Jour. and Rev.*, iv, 428–433, July, 1849. Mosses and Hepatics; v, 324–327, May, 1850, Lichens; vi, 190–199, March, 1851, Fungi.

A catalogue of the natural orders of plants inhabiting the vicinity of the Santee canal, S. C., as represented by genera and species, with observations on the meteorological and topographical conditions of that section of country. *Proc. Am. Ass. Adv. Sci.*, 1850, pp. 2–17.

Description of a new Baptisia found near Aiken, S. C. *Proc. Elliott Soc. Nat. Hist.*, i, 38–39, plate 2. 1859.

Notice of some new and rare phænogamous plants found in South Carolina. *Proc. Elliott Soc. Nat. Hist.*, i, 50–53, 1859.

Report on the fungi of Texas. In Report of Commissioner of Agriculture on diseases of cattle in the United States, pp. 171–174. Washington, 1871.

On the seemingly one-ranked leaves of Baptisia perfoliata. *Proc. Am. Ass. Adv. Sci.*, xx, 391–393, 1871; *Ann. Mag. Nat. Hist.*, ix, 174–175; *Jour. Bot.*, i, n. s., 84–85.

On the relation of the tendril to the phyllotaxis in certain cucurbitaceous plants. *Proc. Am. Ass. Adv. Sci.*, xx, 393–397, 1871.

Some rare southern plants. *Torrey Bull.*, vi, 81–82, March, 1876

Some more rare southern plants. *Torrey Bull.*, vi, 93–94, June, 1876.

Abnormal habit of Asclepias amplexicaulis. *Torrey Bull.*, viii, 87–88, Aug., 1881.

Gordonia pubescens. *Am. Nat.*, March, 1882, pp. 235–238.

The migration of weeds. *Torrey Bull.*, ix, 112–114, Sept., 1882.

Morphology in the tuber of Jerusalem artichoke. *Torrey Bull.*, x, 54–55, May, 1883.

Some North American botanists. Stephen Elliott. *Bot. Gaz.*, viii, 249–253, July, 1883.

A list of the more common native and naturalized plants of South Carolina. In South Carolina Resources and Population, Institutions and Industries. Published by the State Board of Agriculture. Pp. 312–359. Charleston, 1883.

Also, short notices in *Torrey Bulletin*, vi, 88; ix, 23, 128 and 140; xi, 132. A paper on edible mushrooms of this country read before the Aiken Vine growing and Hort. Ass. about 1862–63; was printed in the Charleston newspapers of the day. W. G. FARLOW.

EDITORIAL.

SINCE THE consolidation of the national surveys the government has done nothing for botanical exploration. Millions have been spent in increasing our knowledge of the other riches of our domain, but the plants have been left to private enterprise. It has been claimed that the botanical exploration of this country has been well-nigh completed, but that can only be said by those ignorant of the facts. What botanist does not know that a collector in a single season's work still brings back with him his harvest of new species and increased knowledge of the old? Hundreds of new species are being described yearly in this country. A single contribution, now on our table, describes 160 new species. In the face of all this wealth of undiscovered material, in view of the fact that geology, anthropology, etc., are receiving abundant aid, why does botany lack the fostering care of government appropriation? Money is appropriated for economic botany, but our plea is for the botany of North America. There are many localities not reached by collectors, or reached at such expense of time and labor that but scanty collections are made. The country is settled enough, and botanical interest is so diffused, that exploration could be well parceled out and thoroughly done. The old days of government exploration, when the botanist was compelled to keep pace with a constantly moving pack train, and snatch what he could in his hasty march, are passed, and regions can now be "worked up" in a thoroughly systematic way. The congressional committees on agriculture should be besieged by the botanists of this country to grant an appropriation for botanical exploration, to be under the direction of the already appointed botanical agency, the Department of Agriculture. The herbarium of that department should be enriched by the results of such exploration, until the North American flora is completely represented in our national collection. Such a provision would not only enrich the herbarium of the Department of Agriculture, but all of our larger herbaria, where North American plants are being critically studied. The good feeling of American botanists is such that the possessions of one are the common property of all. Collectors should be sent to unknown or difficult localities to work throughout the season; local botanists in interesting

regions should be employed to thoroughly explore their surroundings; specialists should be sent during the summer to study certain problems. In short, a few thousand dollars from an overflowing treasury could be made to yield an ample return in our better knowledge of one of the noblest and (in a public way) most neglected sciences.

OPEN LETTERS.

A Query.

Can any of the readers of the GAZETTE say whether the spores of Marsilia grown in the United States germinate readily? Having made several unsuccessful attempts last winter to grow the spores, I was informed by Professor Pfeffer that it is difficult to germinate spores grown in Germany, and that all accounts hitherto published have been made from spores imported from Australia. It would be interesting to know whether the same difficulty is met in the United States, and if so, what is the cause. D. H. CAMPBELL.

Tübingen, July, 1887.

NOTES AND NEWS.

IN THE *Journal of Botany* for June Mr. Arthur Bennett presents a revision of the Australian species of Potamogeton, describing one new species.

THE SECOND EDITION of Strasburger's *Botanisches Practicum* has appeared in Germany.

Dr. W. ZOPF has been made Professor-extraordinary of Botany in the University of Halle.

Dr. G. BERTHOLD has been called to the professorship of Botany in the University of Göttingen.

SCARCELY more than a year has passed since the appearance of de Bary's *Vorlesungen über Bacterien*, yet a second and revised edition has been issued by Engelmann.

MR. ROBERT HESSLER, of Connersville, Ind., finds Gaura coccinea growing along the C., H. & I. R. R. near that place. His plant was very bushy and about fifteen inches high.

THE *West American Scientist* for June has an interesting article on the "Oaks of Southern and Lower California" by the editor. It is accompanied by a photograph of live-oaks.

DR. BYRON D. HALSTED, in *Agricultural Science*, has published a paper on the germination of cucurbitaceous plants.

PROF. L. H. BAILEY, JR., of Michigan Agricultural College, wants specimens from all parts of the country of Carex grisea (especially from the south and west), C. triceps, C. arctata, and C. retrocurva.

THE COLONIAL government of Cochin China has conferred upon Dr. Pierre a life pension of 6,000 francs, to provide for the uninterrupted completion of his *Forst-Flora* of Cochin China.—*Nat. Nov.*

DR. EDWARD R. VON JANCZEWSKY has been made Professor of Anatomy and Physiology of Plants in the University of Krakow.

DR. J. C. ARTHUR has accepted the professorship of Botany in Purdue University, pending the establishment of an experiment station at the university. After Sept. 1 his address will be Lafayette, Indiana.

DR. A. DE BARY, of the Botanical Institute at Strasburg, has been appointed Professor of Botany and Director of the Botanical Institute of the University of Leipzig, to succeed Prof. A. Schenk, who retires.

THE BULLETIN of the Calif. Acad. Sci., vol. ii., No. 6, has just been issued. Of botanical interest it contains E. L. Greene's "Studies in the Botany of California and Parts Adjacent—V.," already noticed on p. 346, vol. xi.

DR. N. S. TOWNSHEND delivered an address before the Columbus Horticultural Society at their meeting on May 28, on "Botany in the olden time." The address is printed in the *Journal* of the society for May.

IN THE proceedings of the Newport Nat. Hist. Society, a lecture by Prof. W. W. Bailey on the "Flora of Rhode Island" is published. It is full of interest, and at the close makes a telling appeal for herbaria and botanic gardens.

IN THE *American Naturalist* for June R. C. Stearns describes and figures Araujia albens (a South American asclepiad) as a moth-trap. The moth is caught by its proboscis in searching for nectar, and the more it pulls the tighter the grip, something on the principle of an old-fashioned boot-jack.

REV. FRANCIS WOLLE'S new work on the "Fresh-water Algæ of the United States," which has just been issued, consists of two volumes, one of plates and one of text. It is published by the author, and sold for ten dollars, which is less than half the price of Cooke's "British Fresh-water Algæ," a similar but smaller book.

IN HIS SYNOPSIS of Tillandsieæ Mr. J. G. Baker has completed the first five genera, Sodiroa, numbering 3 species (1 new), Caraguata 12 species, Schlumbergeria 3 species, Guzmannia 5 species (1 new), and Catopsis 9 species (4 new). Tillandsia contains 11 sections, and 19 of its species are described in the July *Journal of Botany*.

DR. GEORGE VASEY, botanist to the Department of Agriculture, has published his third bulletin. The subject is, "Grasses of the South," a pamphlet of 63 pages and 16 plates. Dr. Vasey is doing a valuable work in getting at the needs of various regions and disseminating knowledge concerning the cultivation of grasses and forage plants.

W. D. HAYS' books on fungi seem to be arousing not only the indignation but merriment of our English brethren. The following passage from a review in *Gardener's Chronicle* is a sample: "As for the edible species, perhaps the less said of them the better; possibly some of our very remote, flat-headed, palæolithic precursors might have been able (if their jaws were powerful enough) to masticate woody and leathery Polyporei, accompanied by sheets of dry rot peeled from prostrate logs, with a dessert of shivering, cold and loathsome Tremellas."

THE SIXTH EDITION of Dietrich's *Forst-Flora*, "a description of the most important trees and shrubs for the forester," has just been completed by the publication of the sixtieth *lieferung*. It forms two large quarto volumes, with 300 colored plates. Dr. von Thümen, the editor, is to be congratulated upon the completion of so valuable a work for forestry interests.

THE DISCOVERY, at Kew, of a sensitive labellum in Masdevallia, an Australian orchid, adds to the attractions of that already richly dowered group of plants. An insect alighting on the labellum is lifted up at first slowly, then suddenly as if with a click is caught in the box formed when the lip is closed. In about twenty minutes the lip descends and is as irritable as before.

TEACHERS who wish to demonstrate the presence of starch in vessels will find that it occurs abundantly in the vessels of the leaf-stalks of Plantago major, and also in several other species. Fischer finds the quantity to vary from a few grains to sufficient to completely fill the vessel. Schrenk finds starch abundant in the pitted vessels of the root stock of Aristolochia Serpentaria.

SCIENCE has changed its form and price, the former for the sake of the latter, which could be its only excuse. Having become attached to the old dress, we are not yet prepared to say that we like the new. Time may prove that this was a wise step, and publishers understand their needs better than any one else can, but to an outsider the change looks like a step downward from a high plane.

DIE NATURLICHEN PFLANZENFAMILIEN appears with most commendable rapidity, and the work continues of the same high grade, both in the matter of text and illustration. The eighth part, just at hand, continues the discussion of the Gymnospermæ, begun in the third and fourth parts. The Coniferæ are by Eichler, Engler and Prantl; Gnetaceæ by Eichler; Angiospermæ are also begun by Engler.

MR. WATSON writes that Arabis petræa is to be dropped from our N. Am. flora The Willoughby Mt. plant of the Manual is a true Sisymbrium, S. humile of Meyer, an Asiatic species found throughout British America from Anticosti to British Columbia and Alaska. The Vermont locality is the only one known in the United States. On the other hand, the Greenland Sisymbrium humifusum is an Arabis, and is found in Labrador and on the west coast of Hudson's Bay.

THE ADDRESS of Axel Blytt on "Distribution of Plants," before the botanical section of the Association of Scandinavian Naturalists, is published in the July *Journal of Botany*. His opening sentence is, "The distribution of plants is essentially dependent on climate." His last words are, "I believe the precession of the equinoxes to be a natural cause of such changes in climate, and that the distribution of species is essentially governed by the periodical changes in the earth's orbit."

THE COLUMBUS (O.) HORTICULTURAL SOCIETY held a "strawberry meeting" early in June at Horticultural Hall of Ohio State University, at which papers were read treating of the strawberry from a botanist's stand-point, Prof. W. R. Lazenby; from an originator's stand-point, M. Crawford; from an entomologist's stand-point, W. B. Alwood; from an editor's stand-point, J. J. Jauney; and from a chemist's stand-point, Prof. H. A. Weber. At the close of the meeting the members present considered the strawberries present from the epicure's stand-point, which was no doubt the most delightful stand-point of all.

THE REPORT of the department of botany, British Museum, for 1886, shows 48,111 specimens mounted during the year. The most important collection added was the herbarium of the mycologist C. E. Broome. It consists of a collection of British and foreign fungi, comprising about 40,000 specimens, many of them types. From Edinburgh has been received a collection of plants belonging to Archibald Menzies, who accompanied Vancouver round the world, and whose name is so familiar in our western botany. Our own collectors are well represented among the purchased specimens.

JOS. F. JAMES has a paper in the July number of the *American Naturalist* on "Milkweeds,' throughout which he very curiously confounds the latex and the "sap" of these plants. On p. 608 he says: "Yet, not every plant possesses a sap of such service as that of the milkweed. Serving as a vehicle for the conveyance of nourishment from the roots to the leaves, it carries with it at the same time such disagreeable properties that it becomes a better protection to the plant. * * * There are a great many plants which possess a milky sap, only not so well developed as in the milkweed."

THE LAST NUMBER of *Drugs and Medicines of N. A.* (March) contains the conclusion of the account of Lobelia syphilitica, L. cardinalis, Scrophularia nodosa, Lindera Benzoin, Diphylleia cymosa, Cercis Canadensis and Erechthites hieracifolia. The usual excellence characterizes both text and illustrations. An extensive foot-note gives a sketch of Dr. B. S. Barton, one of the early American botanists of note. It would seem that the claims of a plant which is neither a commercial drug nor used in medicine to a place in this work were somewhat tenuous. This is the case with three of the species listed above.

M. F. CRÉPIN has published (Roy. Bot. Soc. Belgique) "Nouvelles remarques sur les Roses Américaines," called out by Mr. Watson's "History and Revision of the Roses of North America." M. Crépin gives directions to collectors concerning collecting and observation, and then follows with critical remarks upon the species as presented by Mr. Watson. The object is to arouse additional investigation with regard to certain species. RR. acicularis, Nutkana, minutifolia, Carolina, humilis, foliolosa, setigera and gymnocarpa are considered well-known and defined species. RR. lucida and nitida need further observation. R. Mexicana is not pronounced upon, as M. Crépin has not sufficient material; while RR. blanda, Arkansana, pisocarpa, Californica, Fendleri and Woodsii are said to be questionable species.

R. W. RAYMOND'S paper on "Indicative Plants," read before the St. Louis meeting of the Am. Inst. of Mining Engineers, has been distributed. Referring to the uses made of "signs" by practical miners, prospectors, etc., who neglect nothing, for everything is equally important to them, the author thinks that modern science has neglected them too much. Viola calaminaria, the Westphalian "zinc-violet," is described and figured; also, Amorpha canescens, the "lead-plant." Dr. F. Stapff, of Berlin, also reports, since the presentation of the paper, that at Carceres, Spain, the native prospectors locate with surprising skill, in spite of surface gravel, the underlying outcrops of phosphorite, by means of the growth of Convolvulus althæoides. The author adds a fourth case from Montana, where a plant, regarded by experienced prospectors as an indication of silver ore in the soil, turns out to be Eriogonum ovalifolium, which may be destined to be called the "silver-plant."

J. M. JANSE records[1] a remarkable instance of mimetism in the flowers of Maxillaria Lehmanni, from Central America. On the central region of the labellum is a callosity which is covered by a fine yellow powder, which bears an almost exact resemblance to a layer of detached pollen-grains. The author suggests that they are taken for pollen-grains by bees, which devour them eagerly for the large quantity of starch which they contain. * * * The substance in question appears to be an epidermal structure, consisting of the detached, nearly spheroidal, constituent cells of moniliform hairs. Herr Janse believes that this is the first instance recorded of the occurrence of starch in hairs.—*Jour. Roy. Mic. Soc.*

ONE OF THE most important recent announcements is that of the publication of the "Annals of Botany." It is to be an occasional appearance of original papers, well illustrated, on all subjects pertaining to botanical science. The form will resemble that of the *Quarterly Journal of Microscopical Science*, and the fact that it is to be published by the Oxford University Press is a guarantee of fine typographical work. The editors are Prof. Bayley Balfour (Oxford), Dr. Vines (Cambridge) and Dr. Farlow (Harvard). The most prominent botanists in Britain have promised their support, as well as Dr. Gray and Dr. Farlow in this country. It will appear in parts, and the price for each volume will be one guinea.

THE REPORT of the department of agriculture of the University of Minnesota, a single volume covering a period of several years and recently issued, contains two papers by J. C. Arthur on the supposed poisonous algæ, Glœotrichia Pisum (at first called Rivularia fluitans), being the result of observations made in 1882 and 1884. The first of these papers has been already noticed in this journal (vol. viii., p. 266). They deal with the appearance and geographical distribution of the alga, and especially with its supposed poisonous qualities when swallowed by cattle. Prof. M. Stalker, State Veterinarian of Iowa, also makes a report on the latter inquiry; and both observers conclude that the facts do not sustain the view that the alga is detrimental to the life or health of animals.

PROF. THOS. C. PORTER has issued "A List of the Carices of Pennsylvania," a reprint from Proc. Philad. Acad. The authority of the list, aside from the name of a well-known botanist, is attested in the first sentence: "All the species of Carex contained in this list are represented in the herbarium of Lafayette College by specimens from all the counties named, with the single exception of C. Torreyi." If all our catalogues rested upon such a substantial basis they would mean much more than they do. Prof. Porter has long had Pennsylvania botany within the reach of his hands, and in no group has he taken greater interest than in Carices. A good feature is putting the name of the county first in small caps, followed by particular stations in ordinary print, and then the name of the collector in italics. The list comprises 122 numbers, 98 species and 24 varieties.

WHETHER "for the present and for practical purposes a lichen remains a lichen" or not, the evidence seems to be daily growing stronger that a lichen is nothing but a fungus and an alga in a symbiotic relation. Bonnier has not only succeeded in obtaining by artificial culture in a sterilized atmosphere a well-developed thallus in several species of corticolous and saxicolous lichens, but lately has carried these artificial lichens

[1]Ber. Deutsch. Bot. Gesell., iv (1886), pp. 277-83.

to fructification in several cases. The most successful experiments were conducted at high altitudes in the Alps and Pyrenees. Heretofore one of the chief objections to the cultures of Bonnet, Stahl and others has been that they used gonidia and fungus hyphæ. But in the successful experiments of Bonnier the alga used was a Protococcus or Pleurococcus, while those in which gonidia were used under the same conditions failed.

MR. R. A. ROLFE, in *Gardener's Chronicle* (June 11), describes a bigeneric hybrid between Colax (Lycaste) jugosus as male and Zygopetalum crinitum as female. He gives it a new generic name, compounding the two parent names and calling it Zygocolax. He does not pretend to rank it with natural genera, but considers these hybrids as artificial productions and to be treated accordingly. It is questionable whether they should be dignified with new generic names of any kind, as botanists have not yet grown out of the notion that crossing genera should not be considered distinct. It is still an open question how far the skillful manipulation of cultivators should affect the definition of natural genera. The same writer, in a paper before the Linnean Society on "Bigeneric Orchid Hybrids," presents the following conclusions with regard to orchid hybrids: (1) Hybridization may take place, not only between distinct species, but also between distinct genera, or between plants so structurally different as to be usually regarded as such; (2) these hybrids are generally of artificial origin, or accidentally produced, and can not be treated in the scheme of classification either as varieties, species or genera; (3) the possibility of hybridization taking place between species hitherto considered as distinct does not necessarily prove them to be merely forms of the same species; (4) the occurrence of a hybrid between two structurally different genera does not prove the necessity of uniting them in one, nor can such hybrids be arbitrarily referred to either of the parent genera; (5) species and genera will always have to be dealt with in the scheme of classification according to their structural peculiarities and differences, without reference to the possibility of hybridization taking place between them.

THE TARDY development of the ovules of the Orchidaceæ has long been known. Recent studies of Guignard[1] among plants of this order add to the number and throw much additional light on the subject. So slow is the development of the ovules that in some cases six months elapses between pollination and fecundation. Orchids of temperate climates, however, are quicker, the time varying from one to four weeks. Is this because they are more likely to be exposed to untoward influences than those of the equable tropics? The orchids are not alone, however, in this respect. Maury[2] has recently found that several species of Verbascum have rudimentary ovules at the time of pollination, and other plants are already known. Probably when the subject has been further investigated the list will be greatly increased. It would appear from Van Tieghem's and Guignard's researches that the development of the ovule is a sort of hypertrophy resulting from the formation of an excess of nourishment. It is well known that the pollen tubes live in the tissues of the pistil in a parasitic fashion, and if they develop in their growth a ferment, as certain fungus parasites do, there is no reason why hypertrophy should not be the result in the one case as it so commonly is in the other. Here is an interesting field for some American botanist to work, respecting our native plants.

[1] Ann. Sci. Nat. (Bot.), iv, 202.

[2] Bull. Soc. Bot. France, viii, 529. Fide *J. R. M. S.*

DR. ASA GRAY has been triply honored abroad this year during the season of degrees. Not that any titles could add to the wealth of his fame, but they are very pleasant acknowldgements of his great services to the science of botany. The degrees come from Cambridge, Oxford and Edinburgh. The D. C. L. of Oxford was accompanied by the following summary of his personal characteristics: *Moribus suavissimis veritatisque semper quam famæ propriæ studiosior.* The LL. D. of Cambridge came with the epithet *Floræ sacerdos venerabilis*, and the following address by Dr. Sandys (we clip a translation from the *Independent*): "And now we are glad to come to the Harvard Professor of Natural History, *facile princeps* of transatlantic botanists. Within the period of fifty years how many books has he written about his fairest science, how rich in learning, how admirable in style! How many times has he crossed the ocean that he might more carefully study European herbaria, and better know the leading men in his own department! In examining, reviewing, and sometimes gracefully correcting, the labors of others, what a shrewd, honest and urbane critic has he proved himself to be! How cheerfully, many years ago, among his own western countrymen, was he the first of all to greet the rising sun of our own great Darwin, believing his theory of the origin of various forms of life demanded some First Cause, and was in harmony with a faith in a Deity who has created and governs all things! God grant that it may be allowed to such a man at length to carry to a happy completion that great work which he long ago began, of more accurately describing the flora of North America. Meanwhile, this man who has so long adorned his fair science by his labors and his life, even unto a hoary age, 'bearing,' as our poet says, 'the white blossom of a blameless life,' him, I say, we gladly crown, at least with these flowerets of praise, with this corolla of honor [*his saltem laudis flosculis, hac saltem honoris corolla, libenter coronamus*]. For many, many years may Asa Gray, the venerable priest of Flora, render more illustrious this academic crown!" To no one has the degree ever come more worthily. May the prayer for added years be answered! It finds an echo in every American botanist's heart.